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AN ANTI-DARWINIAN CONTRIBUTION.

The Darwinian Theory of the Origin of Species. By Francis P. Pascoe, F.L.S., &c., ex-President of the Entomological Society. (London: Gurney and Jackson, 1890.)

TO avoid any misconception to which the somewhat ambiguous title of this work may give rise, let it be at once stated that this is not an exposition of the Darwinian theory; neither is it a contribution to that theory in the constructive sense, nor does it attempt to deal with the subject in any other than a destructive sense. The author has, in fact, played the same part in the present little book of some 113 pages that he played some years ago in a similar production which is now out of print, and which bore the title "Notes on Natural Selection and the Origin of Species." Mr. Pascoe has hardly placed himself in the position of "counsel for the opposition," inasmuch as he can scarcely be said to plead anywhere against the Darwinian theory; he acts rather as solicitor for the opposition, and by virtue of his special knowledge of certain groups of insects and his general knowledge of other groups of animals, he has been enabled to collate a large number of difficulties and objections which have occurred to himself and to other naturalists. It is hardly necessary to say that many of the most weighty objections raised by the author have been culled from the writings of Darwin himself. The result has been the production of a brief which is valuable as coming from a systematist of recognized authority, and which the counsel for the opposition may and no doubt will make use of.

In so far as the author quotes from other writers, it is difficult to ascertain how far he has himself grasped the Darwinian principles. Where he does appear in his own personality, he leaves some doubt as to whether he really does understand these principles. For example, there is a passage fronting the title-page which is not given as a quotation, and which we may therefore suppose to be the author's own, and which commences with the statement:—

"Natural selection is assumed to depend on a power in every organism—past and present—intently watching every variation, rigidly destroying any in the least degree injurious, and picking out with unerring skill all that in the future, by gradual accumulations, give a better chance in the struggle for life."

It is difficult to see how natural selection can depend upon a power in an organism either present or past; in fact, selection as understood by most Darwinians is an operation from without, *i.e.* external to the organism; and if the author means to imply that the selection is due to an internal agency, then he has failed to understand the spirit of Darwinism. But the whole passage is so ambiguous that the most favourable view that can be taken of it is that when Mr. Pascoe's counsel comes to consider his brief he may be told that the "power in the organism" is used as a metaphorical expression only.

In a few pages of introduction we have a series of quotations, or rather fragments of quotations, from Her-

bert Spencer, Agassiz, Cope, Wallace, Huxley, Flower, Mivart, and Tyndall. These are strung together without any connecting argument, but are presented in a form which is, to say the least, unfair to the authors in question. They are the odds and ends of various passages which would in most cases bear a quite different construction from that which the author intends his readers to put upon them. He quotes from Darwin to the effect that "with all beings there must be much fortuitous destruction, which can have little or no influence on the course of natural selection." Then he adds:—

"It cannot be contended, for instance, that out of the ten million ova of a codfish the few that attain maturity owe their escape to something not possessed by those that perish."

Certainly not; all that Darwinians would contend for is that out of the millions of codfish whose ova do reach maturity, there would be quite a sufficient amount of material for natural selection to work upon. For this agency can obviously deal only with the organism which is already in existence, and if by fortuitous destruction a very large proportion of the ova are destroyed, that percentage is *hors de combat* so far as natural selection is concerned. Moreover, the production of large numbers of ova shows that natural selection has been at work in the direction of producing an increased fertility. This "objection" does not, therefore, appear to be of a very serious character.

After the introduction the real business of the book begins with a brief historical sketch. The statement of the Darwinian theory is composed chiefly of quotations, and in so far as these are Darwin's own words, they are fairly presented. But when the author offers his own summing up, we again meet with that indistinctness of expression which is one of his most unsatisfactory characteristics:—

"Natural selection, then, is assumed to be a power acting by gradually accumulating small modifications, which, in the future, when increased by inherited modifications, is to the 'advantage of each being.'"

Mr. Pascoe's counsel must be left to put this into form. Sexual selection is, of course, not received by the author, but his objections to this part of the theory are very meagre, and he does not refer to any of the recent contributions to the subject, such, *e.g.*, as the masterly series of observations on spiders by George and Elizabeth Peckham. It can hardly be supposed that a writer possessed of such an extensive knowledge of the literature of the Arthropoda, should not be aware of the existence of this work. In connection with the subject of use and disuse, there is a discussion of the question of the wingless beetles of Madeira, which is of importance, because it deals with a group of insects upon which the author is entitled to pronounce an opinion as an expert. Darwin's explanation of this fact is familiar to all students of the "Origin of Species." The "objections" brought against this explanation are thus stated:—

"Mr. Wallace says that the same species, wingless in Madeira, are winged on the Continent. It may be so. Some of our Hemiptera—*Nabis*, *Pithanus*, *Pyrrhocoris*, &c.—ordinarily wingless, are sometimes found in hot summers to have well-developed wings. But the beetles

of Madeira belong, without exception, to European forms, and in Europe few species are habitual fliers, while wingless, or nearly wingless, species are found far inland, in all parts of the world, belonging principally to the same families as those in Madeira (Curculionidæ, Tenebrionidæ, Carabidæ, Lamiidæ, &c.). It is difficult always to determine, without injury to the insect, what may be a wingless species, but in these four families alone it is safe to say that there are a few thousands. In many cases the elytra are soldered together, and have even grown to the sides of the thorax. In some Lamiidæ allied to *Dorcadion*, and probably in some others, there are vestiges of wings. But how is the union of the elytra to be explained? . . . There is a tendency to be wingless in all the insect orders, either in one or both sexes. Neuters, unlike males or females, are always wingless. Barren females are only neuters from want of food."

That is to say, that because winglessness occurs in other insects through other causes than those which obtain in Madeira, therefore this is an "objection" to the wind-selection theory of Darwin. Since Mr. Pascoe quotes Huxley to the effect that "in science scepticism is a duty," I may be permitted to express my scepticism as to whether the author has really grasped the Darwinian explanation. I must confess that to me the reasoning appears to be of the nature of a *non sequitur*, but I have given the extract in full in order that readers may form their own opinion. It is typical of the whole spirit of the "objections" which the author has marshalled against the Darwinians. In the first place, the above statement does not fairly represent Mr. Wallace's views. He says only that *some of the wingless Madeiran species are winged on the Continent*. "In other cases the wingless Madeira species are distinct from, but closely allied to, winged species of Europe" ("Darwinism," p. 105). But the fact that Darwin sought to explain is not that there occur wingless beetles in Madeira, but that such a large number (over 34 per cent.) out of the whole Coleopterous population should be thus deprived of the means of flight. Moreover, the confirmation which this explanation receives from the insects of Kerguelen, which belong to three different orders, and which are *all* wingless, is not alluded to as it should have been in fairness to the argument. Even the "union of the elytra" may disappear as an objection on further consideration, for if, through wind-selection, *or any other cause*, flight became unnecessary or dangerous, it is quite conceivable that the possession of loose elytra would become a source of positive danger to beetles which bury or live among loose stones and the crevices of bark, and natural selection would in such cases bring about the character in question on account of the superior protection from bodily injury which would be thus furnished.

The next writers whom the author puts into his anti-Darwinian witness-box are Dr. Romanes and Prof. Mivart, the latter of whom he considers to be the "most damaging" opponent. Then we have a whole array of Darwin's own difficulties, which are familiar to all who have read the later editions of the "Origin of Species." In his anxiety to pin Darwinians down to the literal statements of their master, Mr. Pascoe has not done justice to, or has wilfully ignored, the later developments of the theory. It is true that Darwin admitted that, unless "many individuals were similarly and simultaneously modified 'rarely single variations could be perpetuated.'"

. . . Variation, however useful, if confined to one in-

dividual, would be 'generally lost by subsequent intercrossing with ordinary individuals.'

But ideas on this head have become broadened to an extent that the author does not seem to be aware of. The whole number of individuals composing any species may at any period of its existence be divided into two portions presenting variations above and below a line of mean variability. Of these portions one must possess characters more or less advantageous so far as concerns the external conditions, and the other portion must possess characters more or less disadvantageous with respect to those same conditions. On the first portion—which may be considered above the mean line—natural selection will act as a preserving agent; on the second portion—below the mean line—natural selection will act as a destructive agent. That is to say, that the whole number of individuals surviving in the struggle for life is supplied from the portion *above* the mean line of variability. The proportion of individuals of this upper portion which survive will depend upon two factors, the intensity of the action of selection for the time being, and upon the range of variability of the species, *i.e.* the height of the extreme variations above the mean line. Mr. Pascoe occasionally gives extracts from Wallace's "Darwinism," but he cannot have properly digested the third chapter of that work, although he quotes from it, if he still considers the isolated fragments of Darwin's writings which he gives above in the light of "objections." It is obvious that many individuals *are* similarly and simultaneously modified, and since the action of selection in the conservative sense is exerted upon individuals above the mean, while the loss by destruction falls upon those which are below the mean, it follows that in the next generation the line of mean variability will be raised, *i.e.* the species will have come into closer harmony with the external conditions, and so on in successive generations till equilibrium is reached.

Among the objections for which the author makes Dr. Romanes responsible is the well-known one about the giraffe:—

"On the converting 'an ordinary hoofed quadruped' into a giraffe, Mr. Romanes observes: 'Thousands and thousands of changes will be necessary.' . . . 'The tapering down of the hind-quarters would be useless without a tapering up of the fore-quarters.' The chances of such changes are 'infinity to one' against the association of so many changes happening to arise by way of merely fortuitous variation, and these variations occurring by mere accident."

I cannot say how far this passage represents Dr. Romanes's views. The latter portion appears to contain a distinct pleonasm, but this is a point of detail, arising perhaps from the author having torn the passage from its context and then dissecting it. But surely it is not essential to the Darwinian explanation of the form of the giraffe that there should have been any "tapering down" at all. The particular advantage which the form of the animal confers upon it is its *height*; the neck has been elongated and the fore-quarters raised for this purpose, and, as far as I know, neither Darwin nor any of his adherents have asked us to believe that the hind-quarters have been lowered. The other difficulty presented in the above passage is one which is very frequently urged, *viz.* the chances against concurrent favourable variations in

complex and co-ordinated structures. To many impartial critics, however, this class of difficulty will not appear so formidable when it is remembered that the complex co-ordinations which are now witnessed are the final results of long series of modifications superimposed from generation to generation through long periods of time. We are not called upon to believe that co-ordinated characters were developed at once by the occurrence of a sufficient number of individual variations having the necessary co-ordination of distinct variations. Mere elongation of neck would be an advantage to an animal occasionally driven to browse upon the foliage of trees. The most rabid anti-Darwinian will not deny that animals do vary in this character. But if an elongated neck is an advantage, natural selection would seize upon it and perpetuate it in the species presenting the necessary variations in this direction. Then, superimposed upon this character would be other modifications, say strength of neck-muscle, which would give its possessors an advantage in reaching up to their food. Natural selection would, in such a case, be acting upon two distinct characters—length of neck and strength of muscle—which are now co-ordinated, inasmuch as they have both been developed for a common purpose. But it is not necessary to suppose that the variations in these two characters have always been “concomitant”; there is no reason for believing that the individuals with the longest necks were necessarily those with the strongest muscles. All that is claimed is, that among the individuals with the longest necks there would be variability in the strength of the neck-muscles, and that muscular strength has thus been superadded little by little to length of neck through successive generations, until the co-ordination which we now observe has been reached. Whole classes of difficulties in the way of accepting incipient “concomitant variations,” which occur throughout the present work and others of a like character, will, I venture to think, disappear, or at any rate become much diminished, if looked at from this point of view.

About one-half of the whole book is devoted to objections of this and other kinds, but the reader, whether Darwinian or not, will be struck by the total want of method with which the facts are marshalled. This is certainly a surprising defect in a writer of such acknowledged skill as a systematist. Large numbers of the paragraphs are bare statements of facts, and their bearing upon the questions at issue is often so obscure that it is impossible to decide whether the author is writing in favour of Darwinism or against it. The first indication of methodical treatment appears about the middle of the book, where begins a short synopsis of the Invertebrata, which occupies from thirty to forty pages, and of which the object is thus explained:—

“The invertebrate classes are so little noticed by writers on the origin of species, that a short sketch of some of their characters and peculiarities may be useful to show their classification and affinities, and to give some idea of their often extremely exceptional forms and modes of reproduction. It will bring into special notice the statements already given in general, and illustrate still further the difficulties of the theory.”

This synopsis is certainly useful as far as it goes, but it contains only the ordinary zoological information (very

much compressed) to be found in any modern text-book, and therefore might well have been dispensed with in a special work like the present where compactness appears to have been aimed at. If the author had only devoted the same amount of space to the detailed discussion of some special classes of difficulties he would have done his own cause more service. To say that “the invertebrate classes are so little noticed by writers on the origin of species” is a distinct injustice to such writers as Wallace, Bates, Fritz Müller, Haeckel, Lubbock, Weismann, Ray Lankester, and Poulton, all of whom have based their contributions to the theory in question, either partially or wholly, on work done in connection with the invertebrates.

After the summary of the characters of the Invertebrata, Mr. Pascoe introduces the subject of colour as a means of protection. He objects (apparently) to natural selection as the effective agency in this case, quoting Mr. Butler to the effect “that no insect in any shape was ever refused by all birds; what one bird refused another would eat.” This objection is based on a misapprehension; no Darwinian has ever asserted that protective colouring is *absolute*, and that the concealment is so perfect that the species thus protected are altogether free from persecution. The utmost that is claimed is a relative advantage. In a similar way Bates’s theory of mimicry is lightly disposed of because “it does not appear that butterflies are anywhere the food of birds.” There is, however, much evidence to the contrary, and the theory of mimicry, moreover, does not require us to believe that birds are the only foes of butterflies. “Another butterfly (*Hypolimnna bolina*) apparently not mimetic,” is a quite erroneous statement, as the female presents one of the most remarkable instances of mimicry known. The leaf-butterfly (*Kallima*) is described as “one of the most perfect cases of mimicry”; but this is rather a case of protective resemblance (Poulton’s “procrystic” group). To say of mimicry that, “after all, the majority of such resemblances are superficial,” is no objection, but would be conceded by the most advanced advocate of Darwinism. The author records a very interesting case of a Queensland beetle (*Saragus*) which is covered with a waxy secretion which is identical in appearance with a fungus (*Isaria*). The bent of the writer’s mind will be gathered from the statement that the fact that the beetle is found on the trees on which the fungus grows is regarded as “the oddest thing” (!). Then he shuts his eyes to the only reasonable explanation, because the beetle belongs to a family which have a hard exoskeleton, and “none of which are likely to be touched by birds.” The reasoning is inconclusive, because hardness of covering is not always a protection, and birds are not the only beetle destroyers. With respect to this whole question of colour adaptation, Mr. Pascoe makes a most astonishing statement: “My experience leads me to the conclusion that, as a rule, animals think very little of concealment.” If this is his experience, it is certainly at variance with that of every naturalist who has observed animals among their natural surroundings.

The concluding portions deal with Weismann’s theory of heredity, instincts, geographical distribution, and other miscellaneous subjects, which are all treated of in the

fragmentary way that prevails throughout. But in spite of any hostile criticism which the work may call forth, it must be recognized that the author has a distinct claim to make himself heard. He is an authority on certain groups of Coleoptera, and is a veteran among systematists who had won his spurs before his present critic knew a beetle from a moth. To attempt to answer all the objections and difficulties which he has accumulated would necessitate a volume. With respect to these objections, some are old, and have already been answered; others are trivial, and require no answer; others answer themselves; others are strained; others arise from misapprehension, as I have endeavoured to show; while others remain as real difficulties, for the solution of which we may have to wait patiently for years. It must not be forgotten, however, that difficulties are not necessarily disproofs. Astronomers tell us that there are many difficulties connected with the motions of the heavenly bodies, but I am not aware that these difficulties have shaken their faith in the theory of gravitation. The man of science accepts that theory, although he might find it impossible to explain why a particular pebble found its way to the top of a particular hill.

In justice to Mr. Pascoe, from whom I have long ago "agreed to differ" on these questions, it must be pointed out that he is, or at least appears to be, an evolutionist. He says so more than once in this work:—"The objections now are confined to natural selection. No objection can be advanced against the theory of descent. A separate creation for each species would admit of no blood-relationship" (p. 107). "We are grateful to Mr. Darwin for having freed science from the bonds of the old theology" (p. 111). It is with the Darwinian form of evolution that he wages war, and if I have availed myself of the opportunity afforded by the Editor of *NATURE* for discussing the contents of this volume at greater length than its bulk might appear to demand, it is because the author is the representative of a large class of systematic entomologists in this country, who hold similar views, and who, by their constant study of and search for minute characters upon which to found "species," have become biased in judgment with respect to the broader problems of modern biology. Writers of this school are in the habit of forcing difficulties upon natural selection which that theory has never professed to deal with. The Darwinian doctrine will not collapse under this last attack, and however much we may differ from the author, it cannot be denied that, as a stimulus to further research, such compilations as that which Mr. Pascoe has produced are distinctly useful.

R. MELDOLA.

THE HISTOLOGY AND PHYSIOLOGY OF GRANITE.

Contributions to a Knowledge of the Granites of Leinster.

By W. J. Sollas, LL.D., D.Sc., F.R.S., Professor of Geology and Mineralogy, University of Dublin. Transactions of the Royal Irish Academy, Vol. XXIX., Part XIV., January 1891. (Dublin.)

THE convenient system of the Royal Irish Academy, by which each paper in their Transactions constitutes a separate part, makes this quarto memoir of

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nearly a hundred pages virtually an independent work. As such it should be possessed by all engaged in serious petrographic research; while to workers in other branches of geology, it will give a clear idea of the patient methods and precise observations by which the history of rock-masses is gradually being brought to light. The merchant and the *connoisseur* of ornamental stones will find little in these pages concerning the broad features of granite, the joints that so often limit the exposed masses, the modes of disintegration, or even the life-history of the Leinster mountain-chain; while the lover of scenery will miss with regret the handsome plates, connecting surface features with geological structure, which adorned the Transactions of Societies at the commencement of the present century. But we would refer such readers to the limitations and restraint shown in the title of the present paper. It is the work of one who appreciates the labours of his predecessors; yet, despite the bibliography which Prof. Sollas has here drawn up, we soon become aware that our knowledge of the familiar rock, granite, is greatly in need of these "contributions." The detailed chemical and numerical observations of Dr. S. Houghton, so freely quoted from by geological writers during the last thirty years, have clearly borne good fruit in Dublin on their natural soil.

The minerals of the Leinster granite are considered in this paper individually. In obtaining the specific gravity of isolated grains, the author ingeniously employs a "diffusion-column," composed of heavy liquid, the density of which increases regularly from above downwards (see *NATURE*, February 26, p. 404). It is found that a column prepared by pouring a less dense upon a heavier solution acquires by diffusion the necessary regularity after standing for some six or seven hours. Minerals of known density are introduced as index-marks, and the density of any layer of the liquid between those at which they float can be read off against a graduated mirror. The convenience of this method, as compared with the ordinary plan of uniformly diluting a sample of liquid for the examination of each separate mineral, will be apparent to all who have worked at the determination of rock-constituents; the purity, moreover, of a group of grains, which have been isolated by the ordinary method, may be ensured by transferring them to the diffusion-column, when some may be found to float above and some below the horizon of the pure material.

We notice that Prof. Sollas uses in his investigations the solution of iodides of mercury and potassium: without being contentious over matters of priority, we must protest against his styling this the Thoulet solution, since its inventor, Mr. Sonstadt, was the first, not only to employ it for the determination of specific gravities, but to suggest its utility in processes of isolation.

The author, when dealing with the biotite of the Aughrim granite, discusses in some detail the constitution of the micas, and graphically represents their molecules in the form of sexradiate rings. Now that the forms of molecules are playing a prominent part in crystallographic considerations, chemists will be attracted by these symmetrical diagrams, which are, of course, mainly speculative. Prof. Sollas himself regards them merely as suggestive; but we can imagine some fascinated student, gazing on graphic formulæ which so skilfully